

DEPARTMENT OF BACTERIOLOGY AND IMMUNOLOGY
HARVARD MEDICAL SCHOOL
25 SHATTUCK STREET
BOSTON 15, MASSACHUSETTS

May 23, 1956

Dr. J. Lederberg
Department of Genetics
University of Wisconsin
Madison 6, Wisconsin

Dear Dr. Lederberg,

As a participant on the program, you are probably informed that I will cover the subject, transduction, at the Johns Hopkins Symposium in June. A rough draft of my manuscript has been completed; however, there are several points that I hope you will have time to clear up for me before the manuscript reaches its final form (by June 15th). These matters relate to the subject of abortive transductions and the formation of motile cells by non-motile strains in general.

My current understanding of these several situations is the following: Spontaneously, on agar medium or in liquid culture, an "event" occurs which results in the production of a motile cell. Motility is inherited in what Stocker ("Bacterial Anatomy") calls unilateral fashion; the flagella are "diluted" until there is but one per cell at which time the single flagellum is passed to progeny in the described fashion. On agar, the initial possession of several flagella enables the cell to swim away from the parent colony; however, the possession of a single flagellum is not enough to endow the cell with the ability to depart from the satellite colony (which, itself, is of small size). Now, in abortive transduction a similar sequence of events takes place. The difference here is in the production at each division of the cell containing the "genetic fragment" of a phenotypically motile daughter cell. This phenotypically motile daughter cell is analogous to that which forms spontaneously in its pattern of inheritance; it may give several motile progeny, but non-motile progeny

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Spontaneously motile bacterium is equal to that one of
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are split off as soon as the number of flagella per cell reaches one (or a sufficiently low number to ^{make it improbable that both cells} allow ~~their~~ chances of receiving a flagellum). ~~improbable~~). Like the satellite clones, division of these clones results in a low enough number of flagella per ~~satellite~~ cell so that motility is impaired in semi-solid medium; thus the "trails" are not branched.

I would greatly appreciate your taking the time to let me know if this interpretation of your (and Dr. Stocker's) data is in keeping with the facts to date. If it is not, or there are other more attractive explanations, a few words on these points would be welcome. Also, I am wondering if various non-motile strains have been "crossed" to recover trails; it would seem that their presence might indicate non-allelism (analogous to a wild-type trans-heterogenote).

Many thanks for your attention to these questions. I hope to see you at the Cold Spring Harbor Symposium, but if not, will look forward to an interesting discussion at McCollom-Pratt. My address after May 30th will be: c/o Department of Genetics, Carnegie Institution of Washington, Cold Spring Harbor, L.I., N.Y.

Very truly yours,

Philip E. Hartman